Wright

The University of Melbourne

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Dear Josh and Esther,

Many thanks for your letter of August 15th. I have been fantastically, but happily, busy since receipt of same and this state of affairs looks like continuing for a couple of months at least. However, this is get-ready-for-next-experiment-week, so I am also catching up on correspondence.

Siri is still blooming and thoroughly captivating - just discovered she can roll over. Astrid is also very fit and enjoying kindergarten tremendously. Mari is free of a cold for the first time in months, must have soaked up all of the Australian respiratory flora by now. And to complete the picture I still have my new lease on life.

No definite news on Canberra yet, but it appears that I will get the job and I am looking forward to it very much. Have not written to Wolfram yet, but will do so this week. I have been struggling with Balassa's articles - still to receive the later one which you mentioned, and which is only available from A.N.U. Canberra. My German is almost completely hopeless, after a long period of disuse and the job would be almost hopeless except for the assistance I have had from a Hungarian med. student who really knows his German. Unfortunately he can't spend much time on it, since he has exams coming up. He seems a very intelligent bloke - with interests much wider than the average med. student too. Am taking an Adult Education course in German to try to overcome my deficiency, and when I hear something definite from C.S.I.R.O. will probably ask them for a complete translation. Such a thing can be obtained here, but the cost is prohibitive for me.

Thanks a lot for Balassa's address. Will write when I know a bit more about the business. Had a reprint from Lary - Staph. - a couple of weeks ago and a letter today. He says that Ruth Korman is going on with the phage transduction in Staph. Where is she now?

I have been asked by Carlton United to give a talk on Yeast Genetics, at the convention of the Australian Institute of Brewing, about a year from now. Do you still have the paper I wrote on Genetics and Yeast Technology for one of your courses Josh? If so, I would like to have it as it would probably save me some work. However, its no tragedy if you haven't got it because I have certainly got plenty of time to prepare for it.

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Maggie Blackwood arrived back a couple of weeks ago, full of pep and is giving a seminar, "New Pathways in Mutation" next Friday, which should be interesting.

Haven't had any time to work on my wall enzyme because I have been running the microbial genetics lab. course for the past three weeks. Quite a good course and all credit to Syd and Bruce for getting it going. That's over now, but will push on with the small clone ---> small colony suppressiveness experiments first. Have done two of these and one has been statistically analysed. Interclonal variance not sig. different from intraclonal variance, but because of high variances we can't be sure. Statistics department has advised me to use more clones and, if necessary, less colonies from each clone, so will start with ten single cells next time and sample as many small colonies as I can from the plates spread after 3 to 4 generations of treatment with euflavine. First time I looked at 5 colonies from each clone at 40 hours (7 to 8 generations on plates) and 64 hours (16 to 18 generations on plates). Second experiment, still being analysed, was not successful - only two of the original five clones grew in the euflavine tubes - acid washed - not rinsed properly? However, I sampled more colonies from each of these clones than in the first run and sampled at 40 and 87 hours (or 20 generations) since the difference between intra and inter clonal variance in the first experiment was more nearly significant at 64 than at 40 hours. (Null hypothesis - no difference. $P_{40} = 0.23$; $P_{64} = 0.1$). Perhaps the difference only becomes significant when suppressiveness is "fully" established, but on another line of reasoning one might expect the whole business to become more randomised with increase in age, in view of the postulated (and necessary) high variability in the hypothetical episomes. Or perhaps I just didn't sample enough clones to begin with. There is a physical limit to the number of colonies I can handle at one time and I thought I had reached it, or exceeded it, in the first experiment. However, things appear less formidable with repitition and, with some technical assistance I think I can handle a 10 x 5 experiment with sampling at 40, 60 and 90 hours. Haven't heard from Ephrussi since he left France, but expect a letter soon.

I was very interested in the Thompson article on episomes which you mentioned Josh. Assume that the difference between the theoretical and observed segregations for full and double bar flies from homozygons bar stocks is due to viability differences and that more than one person has observed the cytological picture described by Bridges (?). Anyway it was an interesting thought and although he didn't postulate a cytoplasmic multiplication phase, I think he rates a mention by J and W. Probably would have got one had he worked at the Institute Pasteur.

If you see Frank Gibson around give him my regards and tell him I will try to drop him a line soon. Mrs. Rubbo is much improved and home again so Syd is in a much happier frame of mind. Not much other departmental news. Nancy still struggling with citric acid - sends her best regards. Bruce and co. working hard on Pseudomonas transduction and waiting for Jan Edgar to return from Stocker's lab. to hop on the Staph. transduction band wagon.

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The weather had been marvellous for about a week until last week-end when it poured. Sunny again today, so it looks as though Spring is really here.

We are watching films of Kruschev's visit on T.V. (next door). The Ruskis are certainly making a lot of propaganda hay lately. What price the moon?

All for now. Hope you are both well and happy.

Kindest regards,

Bob.